it, and it was introduced into public schools, and protected, and nursed, and encouraged by scholarships at the Universities, &c. On the whole it got quite as fair play and more favour than could have been expected. Now it is no longer nursed ; it is left to find its level. It is protected by regulations against the extinctive power of headmasters, and that is all. Meantime, the methods of teaching it are improving ; the supply of teachers is increasing ; the number of scholarships at the Universities is quite adequate to the demand for them ; and the examinations for them are very good. With these favourable circumstances, and a slowly maturing opinion in the minds of most people that education in science is valuable as a part of training, I think we can afford not to be very impatient at the Regulations of the Universities Board, or at the strict neutrality of headmasters when the interests of science are concerned.

JAMES M. WILSON

May I be allowed a few words with reference to some criticisms passed in last week's NATURE on the Regulations adopted by the "Oxford and Cambridge Schools Examination Board" in regard to science?

I fully concur with the writer (Mr. N. M. Watts) that these Regulations and the two papers printed point to a low standard of scientific knowledge in our great schools. It must be borne in mind, however, that the Board does not issue these Regulations as an ideal scheme of school work, but merely intend them to answer the existing state of the case. With the curriculum of any school they have nothing to do, their function being to appoint examiners to such schools as apply for them, leaving the schools free, within reasonable limits, to choose their subjects.

Now granting Mr. Watts' premises that certificates can be obtained *very cheaply* by taking up two sciences instead of Latin and Greek, this would give an impetus to the study of those subjects in schools, resulting in a large flock of scientific candidates.

Whether this has been the case, the following abstract from the examiner's report of last July shows :—

Subjects.	Number of Candidates offering the subject.	Number who satisfied Examiners.	Number who obtained distinction.
Latin	438	308	37
Greek	433	253	35
French	51	34	13
Mathematics ... (Elementary)	455	328	—
English	43	26	9
History	305	234	82
Nat. Philosophy (Mechanical)	21	10	3
Nat. Philosophy (Chemical) ...	28	16	3
Botany	6	4	—
Phys. Geography and Elem. Geology ...	15	7	—

These results show that the number of candidates offering any branch of science is comparatively very small, only seventy out of a total of 461, of whom only thirty-seven succeeded in satisfying the examiners out of a total of 232 ; and of these thirty-seven only six succeeded in gaining distinction. These figures show, I think, that in proportion to the time and attention given to science in the schools examined last July, the papers set were neither unreasonably easy nor difficult. I wish especially to point out that increasing the difficulty of obtaining certificates by help of science would tend as far as possible to exclude science from the school curriculum, while retaining a low standard encourages boys who have gained a certain crude scientific culture in the lower forms, not, as is so often the case, to let it drop entirely on reaching the sixth.

It does seem to me therefore wiser to commend the wisdom than to deplore the ignorance of the compilers of these Regulations, who aimed at testing the soundness of the small modicum of existing knowledge, rather than fixing a standard which would have practically acted as a prohibition of science.

In considering the amount of knowledge to be expected from boys of eighteen, we must remember the time usually devoted to science work. The following will, as far as my experience goes, be not an unfair statement of the case. A boy commences Latin and French at about eight years of age, at the same time imbibing the first ideas of Mathematics in Elementary Arithmetic ; Greek (or German when it is substituted) at twelve or thirteen, and probably Euclid about the same age ; Science seldom, if ever, before fourteen or fifteen. Thus the candidate offering himself for examination at eighteen has given ten years to Latin, six to Greek, and about three to Science, the number of hours in those three years given to Science being certainly less than that given to Greek. This programme would be true of certainly nine-tenths of our public school-boys who offer themselves for the examination, the remaining tenth consisting of boys who at seventeen show a distinct aptitude for Science or Mathematics, and who then drop a large proportion of their classical work and are enabled to devote one-half of their time, or thereabouts, to their special subjects. The complaint might with more reason be urged by the classical boys proper against these specialised boys, who are allowed to gain their certificates too easily. When the necessity arises, the standard will doubtless be raised, perhaps, by a division (similar to that made in the Mathematical Group) into Elementary and Advanced Science, with a provision that only one elementary science can be taken up.

Mr. Watts, in the article referred to, asks, rather contemptuously, whether "the knowledge of the composition of the air, the reasons for belief in the rotundity of the earth, the meaning of the words watershed, dip, &c., is the utmost that can be demanded of a boy of eighteen who has studied science instead of the older well-established subjects of classics and mathematics." I hope I have shown that the standard of the examination papers was not too low for the candidates who offered themselves. With reference to the desirability of the change in our whole system of education to which Mr. Watts refers, I may be allowed to say that there is by no means at present an agreement even amongst science teachers that such a change is desirable. I refrain from opening up this very wide subject, because I feel that the experiment has not yet had a fair trial.

Rugby LINNÆUS CUMMING

THE ORGANIC IMPURITIES OF DRINKING WATER

ON Thursday last Prof. Frankland delivered a discourse to the Fellows of the Chemical Society at Burlington House on the detection and analytical determination of the organic impurities in potable waters. He said that the more his inquiries into the influence of water upon the public health had extended themselves the more had he become convinced of the great importance of this application of chemical analysis to the community at large, contending that, in the interests of the public health, the bringing to perfection of this branch of analysis was worthy of the greatest efforts of chemists.

The two chief objects to be kept in view in the analysis of potable water are, firstly, the discovery of the evidence of *past* pollution by organic matter ; and secondly, the

quantitative determination of *present* or *actual* organic impurity.

The past history of a water is made out chiefly through the mineral products of oxidation which the polluting organic matters have yielded, and which are still present in the water. As these products are innocuous, it is obvious that if all kinds of organic matter behaved alike under the influence of oxidising agents, such evidence of previous pollution might be safely disregarded; but it is almost superfluous to point out that there are wide differences between various kinds of organic matter in regard to the rapidity with which they combine with oxygen; and of all kinds, that which is organised and living opposes by far the greatest obstacles to oxidation. Now the researches of Chauveau, Burdon Sanderson, Klein, and others, scarcely leave room for doubt that the specific poisons of the so-called zymotic diseases consist of organised and living organic matter, and it is now certain that water is the medium through which some, at least, of these diseases are propagated. It is evident, therefore, that an amount of exposure to oxidising influences which may resolve the dead organic matters present in water into innocuous mineral compounds, may, and probably will, fail to affect those constituents which are endowed with life, and Dr. Frankland adduced, as a striking instance of the persistency of the typhoid poison when diffused in water, the outbreak of a violent epidemic of typhoid fever in a Swiss village through the use of spring water, which, after contamination with the poison, had filtered through nearly a mile of porous earth, but which had nevertheless lost none of its virulent properties. As the typhoid poison is always liable to be present in sewage, and as there is no test for it, except its effects upon man, the discovery of previous sewage contamination in potable water ought to be one of the chief objects of the analyst.

The *actual*, or present, as distinguished from the *past*, polluting organic matter of potable water can only be ascertained from the amount of carbon and nitrogen found as constituents of the organic matter present in the water at the time when the analysis is made. The method of performing this operation, known to chemists as the "combustion" method, was fully described to the Fellows of the Society by the speaker eight years ago. Improvements since made were mentioned, and t e following proofs of the delicacy and accuracy of the analytical method were adduced:—

To 100,000 parts of a sample of water, rendered as nearly chemically pure as possible, 1·5572 parts of sulphate of quinine were added. The water was then submitted to the method for determining organic carbon and nitrogen just mentioned. The following data compare the quantities of organic carbon and organic nitrogen thus actually added to the water, with those afterwards extracted in each of two analyses:—

	Present.	Found. I.	Found. II.
Organic carbon in 100,000 parts of water	0·857 part.	0·912	0·904 part.
Organic nitrogen in ditto	0·100 ,,	0·0996	0·098 ,,

To 100,000 parts of a similar sample of water 0·7786 part of sulphate of quinine was added, and the following results obtained on analysis:—

	Present.	Found. I.	Found. II.	Found. III.
Organic carbon in 100,000 parts of water	0·429 part.	0·435	0·442	0·440 part.
Organic nitrogen in ditto	0·050 ,,	0·047	0·048	0·048 ,,

To 100,000 parts of a third similar sample of pure water 0·07786 part of sulphate of quinine was added. On analysis this water yielded the following numbers:—

	Present.	Found. I.	Found. II.	Found. III.
Organic carbon in 100,000 parts of water	0·043 part.	0·047	0·050	0·055 part.
Organic nitrogen in ditto.	0·005 ,,	0·006	0·005	0·006 ,,

The close approximation of the experimental to the calculated numbers is the more striking when it is remembered that the weight of nitrogen *actually determined* in the litre of water used for analysis was, in the last series of experiments, only $\frac{1}{20000}$th of a gramme.

Applied to actual specimens of potable water, the accuracy of the method was tested by the uniformity of results obtained in the following duplicate analyses of the same samples of water:—

Results of analyses expressed in parts per 100,000.

		I.	II.
Thames water as supplied to London ...	Organic carbon	0·280	0·285 part.
	,, nitrogen	0·032	0·035 ,,
River Lea water as supplied to London.	Organic carbon	0·231	0·239 part.
	,, nitrogen	0·042	0·042 ,,
Kent Company's water as delivered in London	Organic carbon	0·054	0·056 part.
	,, nitrogen	0·016	0·017 ,,

But as practical illustrations of the trustworthiness of the process, the speaker relied most upon the results of the monthly analyses of the water delivered by the eight metropolitan companies made for the Registrar-General during the last eight years, and embodied in two large diagrams which exhibited, at a glance, the results of nearly 800 separate analyses. One of these diagrams showed, by means of curves, the mean proportions of organic elements (organic carbon and organic nitrogen) in the waters of the Thames and Lea, and compared them with that found in the deep well-water of the Kent Company. It also showed the rate of flow of the Thames nearly opposite Hampton Court Palace, and consequently near the place where the Thames water companies abstract their supplies. This diagram showed how sharply the distinction between these three waters is drawn by the method of analysis. In no instance did the curve representing the average organic impurity in the Thames approach near to that indicating the like impurity in the deep-well water, whilst the curve of organic contamination in the Lea water intersected the Thames curve but thrice, and the deep-well curve only once in eight years; and even these intersections, when closely studied, were found to be striking illustrations of the delicacy of the analytical method.

The second diagram might be regarded as a magnified representation of the first. In it the curve representing the average organic impurity in Thames water was decomposed into five constituent curves showing the organic impurity in the water delivered by each of the five metropolitan water companies which abstract their supplies from the Thames; whilst the corresponding curve of impurity in the River Lea was split into two, one representing the impurity in the New River Company's water, and the other that in the beverage delivered by the East London Water Company. As deep-well water is delivered to London by one company only, the curve representing the minute impurity in this water was the same in both diagrams.

These diagrams demonstrated how faithfully the analytical results recorded, firstly, the well-known superiority of deep well over river water; secondly, the superiority of the water of the Lea to that of the Thames; thirdly, the variations in the three great conditions which govern the intensity of organic contamination in the river waters, viz., heavy floods, small floods when the river is low, and decay of vegetation in autumn; and lastly, the method has shown itself competent to reveal the finer shades of quality in waters drawn simultaneously from the same source, but treated differently by the various companies who manipulate them.

Against these advantages the process of analysis advocated by the speaker involves more trouble and more careful manipulation than are usually bestowed upon what are called "commercial" analyses, and although these drawbacks ought not to be paramount considerations, where such important issues are involved, yet if any other more simple method existed from which trustworthy quantitative information about the organic matter in water could be obtained, the more troublesome process would cease to have a *raison d'être*.

Such a simple alternative method of determining organic nitrogen, but not organic carbon, is now very extensively used by chemists. It is known as the "albuminoid ammonia" method, and depends upon the fact that, by boiling with an alkaline solution of potassic permanganate, most nitrogenous organic bodies are decomposed with evolution of ammonia. From the amount of ammonia so evolved, the proportion of organic nitrogen is calculated. A critical examination of the results obtained by this method conclusively demonstrates that it is incapable of converting into ammonia either the whole, or any definite proportion, of the organic nitrogen of potable waters. Indeed, this is shown not only by the following instances, but also by numerous others in which known quantities of nitrogenous organic matters of known composition were submitted to the process.

Results of analysis expressed in parts per 100,000 :—

Artificial Waters containing Peaty Matter.

	Organic nitrogen by combustion.	Organic nitrogen by albuminoid ammonia process.
Sample No. 1	·068 part	... ·016 part.
,, No. 2	·042 ,,	... ·016 ,,
,, No. 3	·076 ,,	... ·022 ,,
,, No. 4	1·015 ,,	... ·308 ,,
,, No. 5	1·175 ,,	... ·422 ,,
,, No. 6	·029 ,,	... ·011 ,,
Natural Waters.		
Chelsea Company's water	·058 ,,	... ·011 ,,
West Middlesex Company's water	·027 ,,	... ·012 ,,
Southwark Company's water ...	·061 ,,	... ·024 ,,
Grand Junction water	·031 ,,	... ·006 ,,
Lambeth Company's water... ...	·062 ,,	... ·030 ,,
Artesian well water	·033 ,,	... { ·003 ,, ·004 ,,
Sea water, No. 1	·217 ,,	... ·006 ,,
,, No. 2	·134 ,,	... ·018 ,,

It is almost superfluous to say that any opinion as to the quality of a sample of water, based upon the albuminoid ammonia obtained, must be entirely untrustworthy.

Dr. Frankland summed up with the following conclusions, to which he had been led by the experiments of himself and others :—

1. That the "albuminoid ammonia" process of analysing water affords no evidence whatever of the absolute quantity, either of organic matter, or of organic nitrogen present in potable water.

2. That it does not indicate, even approximately, the relative quantities either of organic matter or of organic nitrogen in different samples of such water.

3. That it affords no indication, either of the presence or of the proportion, of *albuminoid* as distinguished from other nitrogenous organic compounds.

4. That the "combustion" process, though more troublesome, is the only method at present known which affords any trustworthy information respecting the organic matters present in potable waters.

5. That it is the only method which even professes to determine organic carbon in such waters.

6. That the determinations by it of organic carbon and nitrogen are fairly accurate, notwithstanding the very minute quantities of matter dealt with, and that the errors even of a comparatively inexperienced analyst fall far short of the limits which would affect a verdict upon the quality of the water submitted to investigation.

7. That it is the only process which discloses the proportion of nitrogen to carbon in the organic matter of waters, such information being often of prime importance in reference to the origin of the organic matter.

8. That since the improvements which have been made in the mode of evaporating the water to be analysed, the process can now be conducted in any laboratory and with a moderate expenditure of time and labour.

RELATION BETWEEN THE LIMIT OF THE POWERS OF THE MICROSCOPE AND THE ULTIMATE MOLECULES OF MATTER[1]

THE subject which I have selected for my address is the relation between the limit of the powers of the microscope, and the ultimate molecules of organic and inorganic matter. I think I may at all events claim for this question sufficient novelty. Until the last few years the subject could scarcely have been attempted, and even now so many necessary facts are imperfectly known, that nothing more can be done than to fill the gaps with plausible assumptions. This necessarily imparts more or less of a speculative character to some of my remarks ; but it appears to me that in his annual address the president of a society cannot do better than endeavour to point out the general bearings of what is already known on some great question, even if for no other object than to prove the need of further inquiry.

Though fully impressed with the imperfect state of our knowledge, yet, even now, the facts are sufficiently definite to indicate, if not to prove, the existence of as wide a world of structure beyond the limit of the power of the microscope, as what has been revealed to us by it is beyond the powers of the unassisted eye.

I propose to divide my subject into three heads—

1. The limits of the power of the microscope.
2. The size of the ultimate molecules of organic and inorganic matter.
3. Conclusions to be drawn from the general facts.

In considering the limits of the power of the microscope, I shall assume that the instrument itself is perfect, and that the invisibility of the objects examined is in no way dependent on the want of the necessary characters. The point to which I particularly wish to direct attention is the limit of visibility depending on the constitution of light, beyond which light itself is of too coarse a nature to allow of our seeing objects distinctly defined. This question has been treated of in an admirable manner by Helmholtz in the jubilee volume of *Poggendorff's Annalen* (1874, p. 573). The conclusion to which he arrives is that the limit depends on the confusion in the image due to the bright interference fringes overlapping the dark outlines of the object. This limit varies directly as the wave-length of the light, and inversely as the sine of half the angle of the aperture of the object-glass when illuminated by means of a condenser of equal aperture. According to this principle the limit for dry object glasses of 60° aperture is, roughly speaking, about equal to the wave-length of the light, and for the largest possible aperture equal to $\frac{1}{2}$ the wave-length. In the case of immersion object glasses of the same aperture, the limit is about $\frac{3}{4}$ of that for dry.

On comparing this theory with the results of observation, the agreement is very striking. It indicates exactly the same law for the increased defining powers of lenses of large aperture, as has been determined by experiment, and gives for the theoretical limit of distinct visibility $\frac{1}{80000}$th part of an inch, which is exactly the same as the mean of the experimental determination of a number of the most skilful microscopists. It also shows why in the case

[1] Anniversary Address of the President of the Royal Microscopical Society, H. C. Sorby, F.R.S., &c. Abstract by the Author.